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ABSTRACT

The activities involved in coding conversation sequences and the lack of attention given to their systematic validation within the field of speech communication are discussed in this paper. The paper first reviews briefly the assumptions underlying relational coding and the implications of viewing these activities as practical accomplishments. It provides a theoretic rationale that draws attention to the need to study the cyclic behavior of groups as they sequence themselves conversationally within and across talk. A study that employed these methods of analysis is discussed, as are the findings and their implications for understanding relational development in group systems. Finally, the paper offers a reflexive critique of the methodological assumptions and procedures employed to study these particular group systems. (FL)

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A REFLEXIVE ANALYSIS OF CONVERSATIONAL SEQUENCING IN GROUP SYSTEMS

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> Human beings have a tendency to involve themselves in sequences of cumulative interaction...The essence and raison d'etre of communication is the creation of redundancy, meaning, pattern, predictability, information, and/or the reduction of the random by "restraint". (Gregory Bateson, 1972, pp.115, 130-131)

> It is how you accomplish the rational demonstration of your inquiries that is of interest as a matter of study... That is the phenomenon. Your inquiries are practical accomplishments...You know that what we take for granted is, of course, interesting. But really we ought to get around to it sometime and get a good look at it. (Harold Garfinkel, in Hill and Crittenden, 1968, pp.194, 199)

The alternatives for analyzing sequences of communicative activities are infinite. Guiding the focus of this empirical and reflexive investigation, however, is a consideration of the interface of two seemingly juxtaposed views: The Interactional View and Ethnomethodology.[1] The former, an increasingly prominent research orientation within the communication discipline, especially in the form of relational coding research (e.g. Millar and Rogers, 1976; Ellis, 1979; Fisher and Beach, 1979), is reflected in Bateson's quote. The latter, having emerged as a result of the work of left-wing and/or radical sociologists (e.g. Garfinkel, 1967; Hill and Crittenden, 1968; Douglas, 1970; Turner, 1974), is represented in part by Garfinkel's statements. How might two such explanatory frameworks be employed concomitantly in a study of interactional sequences? I suggest within the present inquiry that ethnomethodological assumptions can lead to a presuppositional questioning of the current status of interactional research. By contrasting one with the other, issues that have been taken-for-granted in the relational coding of sequential behavior can be displayed and examined. Whereas it should come as no surprise

that inherent in the relational coding of conversation are overlooked principles of reliability and validity, since all research orientations rest on unexplicated presuppositions (see Peters, 1976), it is a more critical stance to imply that knowledge claims in the form of empirical results are highly suspect.

Within the present investigation, I ground my argument by reporting an empirical analysis of cyclic behavior in small decision-making groups. Let's assume that the study itself be viewed as a sequence of events, i.e. as meaningful data, rather than merely focusing upon the statistical findings concerning how same groups structured their conversations within and across two different group tasks. Group behavior was tapped by examining how members collectively displayed relational definitions (i.e. $\uparrow^{+}, \uparrow^{-}, \rightarrow, \downarrow^{+}_{-}$) while conversing (see Ellis et al., 1977). Moreover, relational control functions were submitted to Markov chain analyses. These statistical tools can be used to assess time-dependant probability structures of interaction (see Fararo, 1969; Hewes, 1975; 1979). The need to conduct such a study is addressed in detail, in that previous research on phases of group development can be extended by such an orientation. Yet when a reflexive dimension is added to the actual doing of this study, it is seen that serious questions can be raised as to how these practical accomplishments--and the rational properties constituting these activities--are problematic. Specifically, I discuss the nature of coding activities and the lack of attention given to their systematic validation within our field. As will be argued, the empirical findings of this report, or any investigation based on the relational coding of talk, make sense only in light of how coding activities get accomplished.

I proceed in the following manner. First, assumptions underlying relational coding are briefly discussed, as are the implications of viewing these activities

as practical accomplishments. Second, a theoretic rationale is provided drawing attention to the need to study the cyclic behavior of groups as they sequence themselves conversationally within and across tasks. The methods of analysis are discussed, as are the empirical findings and their implications for understanding relational development in group systems. Finally, a reflexive critique of the methodological assumptions and procedures employed to study these particular group systems is offered. Combined, these interrelated discussions are grounded in the belief that the refinement of communication inquiry can occur by studying how we study, i.e. by transforming how we accomplish our research activities (as a resource) into a topic of investigation. In so doing a more precise reflexive linkage can be made between what we do as social scientists, how we do our work, and the impacts these practical accomplishments have on our understandings of communicative phenomena.

I. Contrasting Views on Relational Coding Research

Within the communication discipline, relational coding inquiries are represented by such work as Rogers and Farace (1975), Millar and Rogers (1976), Ellis (1979), and Fisher and Beach (1979). A growing host of other research reports also exist (see Rogers-Millar, 1979). It has been referred to as "relational communication" (Parks, 1977), and discussed in terms of its rather intimate ties with the Interactional View at Palo Alto (see Wilder, 1979).[2] Among other assumptions, of major concern is how sequences of actions are pragmatically tied. Inherent in the notion of behavioral pragmatics (see Watzlawick, Beavin, and Jackson, 1967) is the recognition that how people behave becomes functional within the rule-sets of the interactional setting, as forseen by identifying patterns (redundancies, restraints) of symmetry (functionally similar responses, e.g.↑↑,↓↓) and complementarity (functionally dissimilar but compatible responses, e.g.↓↑,→↑) (see Bateson, 1972, pp.233-234). Watzlawick et al. (1967, Ch.4) have referred to the redundancies among these

sequential structures as interactional "punctuation", i.e. ways in which chains of messages are discernable as holistic units. Fisher (1978, pp.206-207) has noted that the assumptions of interactional punctuation are consistent with information theory (e.g. Wiener, 1954) conceptualizations of how acts are connected probabilistically. The emergence of communication systems, a consideration relevant to the present study on group development, is dependant upon how patterns of acts serve to constrain one another as a developmental course of events (also see Teune and Mlinar, 1978). Following Bateson's (1972; 1978) concerns with understanding systemic wholes rather than isolated component parts, discovering what interaction sequences are and how they are organized is like attempting to "map out a territory" of communicative behavior, i.e. to explicate patterns and patterns of patterns.

Surely the goals of pragmatic research are worthwhile, but how these goals are empirically attained is of equal concern to this investigation. For example, the utilization of relational coding schemes rests upon the assumption that in the process of tapping utterance functions, predetermined category types are operationalized by coders who are trained to become reliable and efficient interpretive agents. Their task is to code each act in relation to the previously occurring act, so as to preserve the interdependance and coherence of the talk. This leads to assessments as to how individuals jointly define their relationships over time. But how are these coding activities accomplished? What types of "relational definitions" ensue? What problems in training coders exist, how do coders come to operationalize recognition rules of the coding system, and what are the assumptions of social reality inherent in the coding of conversation into predetermined category types? These and other questions are basic to the foundational validity of relational coding, and are in need of critical inspection.

It is in this sense that Garfinkel (1967; in Hill and Crittendon, 1968) argues that research activities themselves are meaningful according to their

practical accomplishment. In the process of scientifically generating explanations about social phenomena, researchers enact sequences of procedures that lead to epistemic claims (and/or ontological assessments). Underlying the conducting of studies of human interaction are series of interpretive moves by researchers to make sense of the situations in which they find themselves. How these moves are done determines the ordering and arrangement of data, and ultimately the rendering of accounts about the phenomena being investigated. Rather than ignoring or only givin lip-service to these activities, a critical evaluation of observation and measurement procedures can shed reflexive light on the conducting of social inquiry. Those procedures, having been treated as only resources or tools, are transformed into topics worthy of investigation; they are not merely taken-for-granted or assumed unimportant. Among the intended results of such reflexivity is an increased awareness of the methods employed in the analysis, and, hence, of their impact throughout the explanatory process. Further, knowledge claims become grounded in their modes of production. The overriding goal, of course, is a more rigorous and precise exploration of the social world in which we reside.

These and similar issues have been raised by Hawes (1978) in his discussion of the role of reflexivity in communication research, O'Keefe, Delia, and O'Keefe (1978) in their critical stance on how interaction analysis techniques serve to distory conversational elements, and Beach (1978) as he argues that the enactment of coding routines is a complex hermeneutic task, subject to diversified bias and error when studying conversation. However, the next step is to integrate these arguments with actual empirical studies utilizing relational coding procedures, i.e. to display what impacts (if any) these queries have on explanations of communicative activities. I now turn to the description and reporting of a study that presently serves such a purpose.

II. Studying Cyclic Behavior in Group Systems

As noted, the empirical portion of this investigation concerns how same group systems pattern their interaction within and across two different group tasks. The need for such an inquiry is predicated on several assumptions. To begin, the use of discussion tasks in social science research is typified by the employment of both single task designs, as well as multiple task designs across different group systems (see Bailey, 1978). Questions can thus be raised as to whether or not same groups recycle through their preestablished phases of interaction when faced with a different task. This presupposes, of course, that phases of some sort are established during initial group performance.

Daily interaction involves the need to work and play with same groups for multiple reasons. Yet attention has not been given to how these shifts in purposes and settings influence conversational sequencing. A consideration of existing group research that has dealt with the nature of group tasks supports this claim. Roby and Lanzetta (1968), Hackman (1968), Hare (1976, Ch.4), and Bochner (1974) have argued that task variation is in need of empirical attention, and for purposes of this study can aid in clarifying the cyclical nature of group decisioning. Questions can thus be asked: What occurs during and after initial task accomplishment?; What kinds of cyclical stages recur, and how do they vary?

Also, relational control dimensions have not been previously utilized to tap developmental processes in groups, and can extend accounts of shifts in the group process. It is assumed, as mentioned previously, that messages may be coded in terms of how participants mutually regulate and shape group structure. Conclusions can then be drawn as to how members control the flow of interaction and overall progression of the group. Communicators are constantly organizing their environments, and in so doing sequential patterns often emerge that are relationally

indicative. The following empirical review of previous group development research further substantiates the potential usefulness of a relational coding orientation to group decisioning.

Researching Developmental Stages

A series of generalizations can be provided about the research conducted on stages of group development. While I do not intend to provide a detailed history of this literature which is available elsewhere (e.g. Shaw, 1971, Ch.4; Hare, 1976, Ch.4), It will be useful to discuss the findings of several studies and the variations of methodologies employed. This will provide a backdrop for displaying that cyclical development across tasks has been overlooked, and how such task behavior can be functionally and relationally coded prior to being statistically assessed for probability structures.

Within this literature, several terms are used interchangably in describing group development, namely those references to phases, stages, change processes, interactional shifts, and sequential variations of group behavior. Underlying these characterizations there does appear to be a relatively constant and general definition of group movements, as summarized by Bales and Strodtbeck:

> By "phases"...we mean qualitatively different subperiods within a total continuous period of interaction in which a group proceeds from initiation to completion of a problem involving a group decision. (1951, p.485)

While there are no hard and fast empirical findings concerning decisioning stages--no specific phase hypotheses have received undisputable empirical support--strong similarities do exist across selected phasic studies. For example, Bales and Strodtbeck (1951) note that group members tend to orient themselves to one another and the task at hand begin to critically evaluate the situation by expressing contrasting views on the subject, and eventually take control by generating alternative strategies for action. As groups progress through these phases, they also discovered an increase in positive and negative reactions by individuals. Similarly, Bennis and Shephard (1956) suggest that as individuals face problems

concerning the distribution of authority and intimacy, two major phases become enacted. Their first proposed phase of dependance-authority describes "...group members' orientations toward authority, or more generally toward the handling and distribution of power in a group." (p.416). Once these issues have become somewhat stabilized, assessments are made by members about the shared goals of the group. This leads to the second phase, interdependance-personal, whereby attention is given more directly to personal relations among individuals. This preoccupation provides an atmosphere more conducive to organizing task efforts. In their discussion, Bennis and Shephard (1956, p.417) write that both Schutz (1955) and Bion (1948a; 1948b) proposed similar stances on group development. Although Schutz (1955) refers to basic components of group compatibility, and Bion (1948a; 1948b) discusses emotional aspects of group operation, each also determined that movements beyond orientation to more personal and controlling stages were typical patterns in the groups they observed.

Added to these perspectives are those conclusions drawn by Tuckman (1965) in his thorough review of over fifty developmental research efforts. He assumed the task of proposing a hypothetical phasic model that would adequately reflect the results of these inquiries. This led to four generalizable phases that, in Tuckman's (1965, p.396) opinion, "...would seem to withstand the test of common sense as well as being consistent with developmental theory and findings in other areas." These phases are forming (indicative of orienting behavior to group discussion), storming (a negative reaction to task demands mixed with a diminishing of group "newness"), norming (the emergence of cohesiveness and the generation of behavioral standards for dealing with the situation at hand), and performing (whereby members' roles become functional in channeling group

effort toward supportive completion of the task). Of interest here is that Fisher (1970) discovered that groups progress in a fashion almost identical to those phases described by Tuckman (1965). He concluded that shifts in group process tend to follow a pattern of orientation, conflict, emergence, and reinforcement in their verbal task behavior.

The similarities across these findings suggest that groups tend to move beyond orientation stages to more personal and productive phases. However, care should be taken to not interpret such results too literally. Each of the aforementioned authors qualifies the generalizability of their studies according to several basic characteristics of decision-making groups. Not only do members vary as to the commonness of their backgrounds, personalities, expectations toward the group, and interactional styles, but purposes and duration of interaction are not consistent for all groups. Bales and Strodtbeck (1951) restrict their conclusions to groups working on "full-fledged" problems, thus "The type of phase movement is not held to be universal in an empirical sense." (p.485). And Bennis and Shephard (1956) confine their views to human relations and self-study groups. They state that an understanding of group movements must in part be determined according to "...the particular constellation of personalities assembled.", as well as "...a given set of environmental conditions." (p.416). Moreover, Fisher (1970) writes:

> I would be among the first to suggest that the four phases of orientation, conflict, emergence, and reinforcement will not necessarily be present in all task oriented small groups. Certainly, task groups subject to external legitimate controls would modify the "natural" context characteristic of the groups studied and might consequently affect the groups interaction processes. Then, too, permanent groups might deviate from the four-phase pattern in subsequent task performances. The nature and extent of such differences point to the need for further research. (p.65)

Fisher's (1970, p.65) conclusion concerning subsequent task performance highlights a general aspect of developmental work noted previously: This research is typified by phasic analyses within meetings and across discussion sessions

on the same task. Empirical claims of how same groups adapt to differing task situations is an overlooked issue, and this state of affairs raises questions as to whether or not small groups recycle their interaction stages upon the completion of one task and the beginning of another. While the literature I have reviewed here might suggest that groups would reenact decisioning stages with a new task, i.e. members would have to collectively orient themselves to the nes problem, generate guidelines to deal with the task, and move toward controlling the situation at hand, no cumulative knowledge exists upon which such a claim can be grounded. The cyclical nature of cross-task interaction for same groups is thus unknown.

A consideration of the methodologies employed to tap group process reveals an additional need for research, and is also addresed in the present study. Compared to past group development research, a relational coding orientation that is statistically assessed probabilistically offers a more rigorous interpretation of how members self-regulate their conversations interdependantly. This becomes apparent when, as noted by Psathas (1960), communication patterns have frequently been identified by dividing the total number of acts into equal time periods (phases), and then calculating the absolute frequencies of acts per phase. This procedure has also been employed across acts per meeting. By ranking frequencies and noting the distribution of acts by percentage rate, arguments are offered as to "phase movements" (Psathas, 1960, p.185-186). It has been this general procedure that has constituted the work by Bales (1950), Bales and Strodtbeck (1951), Mann, Gibbard, and Hartman (1967), and Mills (1964). It seems that while a good portion of group development researchers "...have been associated with category systems for the observation of interpersonal behavior which could be used to test or illustrate the theories." (Hare, 1976, p.88), an even greater proportion of investigators have generated empirical claims of group movement from frequency (percentage) analyses of acts.

It would be fruitful, therefore, to move beyond defining phases according to the mere distribution of categories, toward an explication of group movement grounded in levels of organized complexity (see Fisher, Glover, and Ellis, 1977). Focusing upon probabilities of recurrence can offer insight as to the interstructuring of emerging behavioral events that research based on distribution assessments cannot. As individual's form collective structures (see Weick, 1969), interstructured behaviors form the basic elements of social organization. The emergence of patterns thus determines the processual nature of the interaction. Fisher et al. (1977) write:

> Since communication occurs in time as acts or events which are sequentially ordered, organized complexity of the interaction becomes measurable only in time...Organized complexity, then, resides in the probability of recurrence of behavioral acts or sequences within the ongoing interaction pattern. (p.232)

Here it is seen that clusters of behaviors (in the present study, relational codes-as-data) serve to constrain one another in sequence, and these patterns characterize the relationships among participants. Over time, these relationships are subject to variation, since potentials for the creation of systemic variety are omnipresent (see Teune and Mlinar, 1978). Moreover, working within a social systems framework provides additional freedom to locate systemic change within the definitions of relationships among members. Hos individuals engage themselves conversationally is indicative of the groups' reactions to a particular task, as evidenced in the interactional patterning of the group system.

Expected Results

Given the present focus upon how same group systems pattern their conversations within and across two different tasks, two major results

were expected. These expectations were, of course, only speculations determined prior to analyzing the empirical outcomes of this report.

First, relying upon past group development research, whereby shifts within and acress meetings have consistently been found to exist, it could be predicted that relational development will occur within the performance of a task. The similarities in findings seem to indicate that groups move beyond orientation phases to more personal and controlling stages. This first expectation is predicated on these rather consistent results.

Second, since no research has been conducted on group cycles across tasks, whether same groups will reenact their decisioning sequences across two tasks constitutes an overlooked empirical question. While the performance of a second task presupposes that group members share an interactional residue, they on the other hand also have to reorient themselves to the new task and behave in manners leading to its accomplishment. Therefore, I assumed that stages in the second task would not significantly differ from those of the first task.

Method

Four groups were drawn from a small group decision-making course at a large midwestern university. Three of these groups contained five persons, while the fourth was comprised of six individuals. At my request, students were allowed to form their own groups at random.

During the class period following group formation, I instructed groups about the first of two tasks they were to engage in, as follows:

> Task #1: As a group, it is your task to discuss and research a social problem, generate alternative ways to solve the problem, and write a paper concerning your findings and solutions. You will have a maximum of one week (three class periods) to complete the task.

Each group was also requested to audio-record their own meetings for subsequent analysis in the class, and checks were made to insure recording quality.[3] Players and cassettes were provided.

Each of the four groups completed the first task within one week, recording two extended sessions a piece (varying from one to one and one-half hours per session). The additional time spent on the task was due to individual reading, writing, and accumulating other material (e.g. through interviews, taking field notes) to bring to the group meetings.

The second task began one week after the completion of the first task. This task was more self-analytic in scope, as described below:

> Task #2: Listen to your recordings of the first task. Take notes concerning what you consider to be characteristics of your group conversations, and how your group decision-making developed over time. You will be required to present your insights and conclusions to the class.

One important qualifier was added to these directions: I requested that they not discuss their individual reactions to the tapes of the first task with one another until all recordings/meetings had been heard. This was necessary because if the tapes of the first task were stopped every few minutes (or less), for example, the recordings of the group comments would be sketchy and disjointed. Rather than deal with this potential problem, and to better insure longer discussions among group members on the second task, groups were asked to discuss and synthesize their notes after the initial tapes were heard in their entirety. This procedure presented no problems to the groups.

These steps for the second task led to two extended meetings for three of the groups, and three meetings for one group. These meetings were also recorded via the same recording procedures employed initially.

Combined, the two aforementioned tasks produced audio-recordings of four groups across two relatively unstructured tasks, although the tasks were different in scope: The first task focusing upon an issue external to the group (i.e. a social problem); The second task being more self-analytic and intrinsic to the group (i.e. make sense of your groups' development during task one).

Groups one, two, and three had thus accumulated four recordings each across two tasks, and group four had accumulated five. (see Figure One).

Relational Coding Scheme and Training of Coders

The scheme employed was Ellis et al's. (1977) REL/COM. It includes five control loadings to discriminate among message types, as follows:

1. Dominance (↑+) -- Attempt to restrict severely the behavioral options of the other.

2. Structuring (↑-) -- Attempt to restrict the behavioral options of other, but leaving a variety of options open, e.g. the option to disagree. Attempt to control the flow of interaction.

3. Equivalence (→) -- Attempt at mutual identification. Interactional modes which do not seek to control or acquiesce to other's control.

4. Deference (↓-) -- Willingness to relinquish some behavioral options to other while retaining some choice of options. Deference is "following" behavior which relinquishes the control of the interaction.

5. Submissiveness (↓+) -- Willingness to relinquish behavioral options to other while retaining little choice.

Three graduate students agreed to serve as coders. The initial training session dealt with a reading of the coding manual, clarifications of coding instructions, and practice coding.[4]

As a group, we discussed each category description in depth, as well as how the five categories work in relation to one another. Once completed, we turned first to the coding examples included in the coding manual. Each of these was discussed among the coders. Next, we turned directly to an audio recording of a small group I had used for other research purposes to practice coding. This allowed the coders to begin functioning as a unit whose goals were to: 1) Operationalize the recognition-rules of the system in identifying utterance types; 2) Discuss problematic utterances and situations, and the decisions needed to be made in assigning codes to them; and 3) Compare results with one another, working out any problems that emerged in this process. Several hours

were spent on these procedures to strengthen the liklihood of attaining high reliability coefficients.

Utilizing Guetzkow's (1950) formula for categorizing reliability, an acceptable (.80 or above) level of agteement across coders, across one hundred and fifty acts, was needed before actual coding could begin subsequent to a termination of the training session. The first reliability check rendered a .84 level of reliability ($p < .01$), a sufficient coefficient to allow coders to begin coding the tapes of the four groups across two tasks. These tapes were randomly distributed across coders Midway through this process, an additional coding check was made to assess possible coding decay. This check led to a .82 level of reliability ($p < .01$), suggesting that coder agreement was maintained beyond the minimally accepted level.

Description of Markov Analyses

The use of finite stochastic models vary from the relatively simple analyses that have prevailed in group development work, i.e. they complexify their treatments of social interaction as compared to: 1)Determining frequecies of behavioral occurrence for each category; 2)Analyzing these frequencies to discover the proportion of interaction each category represents; and 3)Concluding that group movements may be deciphered by these techniques (see Bales, 1950; Bales and Strodtbeck, 1965). In contrast, the mathematical meaning of a stochastic process is to analyze probability-based processes across strings of codes representing discrete, mutually exclusive, and exhaustive categories (Hewes, 1975). To determine how these categories constrain one another in sequence, transition probabilities are computed between utterances. Each utterance can be said to add to interactional structure according to how they induce probability functions for following utterances. Fararo (1969, p.248) describes the Markov modelling of these processes "...as a map associating a function of time with each outcome."

The predictive assumptions of these models, as displayed in matrix forms, are thus based upon probabilistic structures rather than mean-value estimates (Hewes, 1975), and can be used to model decisioning activities (see Hawes and Foley, 1976).

In determining the derivations of these probability structures, the model must be tested against three assumptions: order, homogeneity, and stationarity. Each assumption serves as a null hypothesis for the study.

The assumption of order concerns how any given code in sequence is best predicted from the previous one, two, or nth codes. Discrete Markov chains are based on a first order criteria: Each code is most dependant upon the previous code for its sequential ordering.

The assumption of homogeneity is a test o whether or not population subgroups vary in their probability functions. If subgroups vary in their matrices, the data become heterogeneous and can therefore be contrasted with one another for differences in relational patterns. If not, the null hypothesis is not rejected.

A Markov model may be considered stationary if probability functions do not vary significantly over time. In studying relational control processes within and across discussion tasks, this implies that significant shifts in message structures would not occur across varying time periods. However, the control decisioning becomes nonstationary if, when transition matrices are conputed over shorter periods of time, they appear statistically independant of the composite matrix. If so, change in message structure has occured; variations in relational control are present.

These applications are one means of studying communication systems. Data emerge from the functional coding of turns into predetermined category types. Change among these temporal codes is conceived as shifts in probabilistic structure. Thus, sequential structure is conceived as redundancies of acts through time. These techniques provide procedures for pattern identification, hence, systemic explanations of group conversations.

Statistical Assessments

In applying Markov assumptions to the present study, it was necessary to examine how four groups evolved within and across two different tasks. Figure One displays the total number of acts generated by these groups for each meeting in each task, the total number of acts for each group, and also serves as a reference for determining what analyses needed to be computed for testing the assumptions of order, homogeneity, and stationarity.

Figure 1
Task Design and Number of Acts
Per Meeting, Per Task

	Group 1	#Acts	Group 2	#Acts	Group 3	#Acts	Group 4	#Acts
Task 1	Mtg 1	691	Mtg 1	354	Mtg 1	410	Mtg 1	146
	Mtg 2	513	Mtg 2	369	Mtg 2	380	Mtg 2	110
Total Task 1:		1204		723		790		256
Task 2	Mtg 1	710	Mtg 1	619	Mtg 1	445	Mtg 1	251
	Mtg 2	348	Mtg 2	247	Mtg 2	276	Mtg 2	234
							Mtg 3	90
Total Task 2:		1058		866		721		575
Combined Total (1&2):		2262		1598		1511		831

Two preliminary tests were needed before determining actual group movements within and across tasks. First, sequential order was calculated to discover how utterances were probabilistically structured, i.e. what length of Markov chain the subsequent analysis would proceed from. Results indicated that there was

a failure to reject the null hypothesis, as displayed in Table One. Therefore, the interaction was assumed to be structured on a first order basis: Each act was sequentially tied to the previous act in the interaction structure, and only these structures exceeded the assumption of random distribution.

Table One

Level of Markovity*

		Group 1	Group 2	Group 3	Group 4
Task 1	Mtg 1	59.84	63.82	69.81	13.25
	Mtg 2	35.13	32.01	46.62	27.40
Task 2	Mtg 1	87.31	27.60	65.70	38.41
	Mtg 2	36.50	43.19	54.74	6.77
	Mtg 3				5.79

* Second Order, 80df, critical value=116.3

The second preliminary test was for homogeneity (see Kullbach, Kupperman, and Ku, 1962), which assumes that subgroups do not have different probability structures from the composite of all subgroups. A "subgroup" in this case was the probability ordering of each group for each task, and these matrices were compared to the composite matrix for all subgroups. Groups were found not to be homogeneous (60df, cv=225.15, $p < .01$), and these differences required that groups had to be analyzed separately. The null hypothesis was therefore rejected, since significant variations in probability functions were present.

Assessing Temporality: Stationarity Tests

Once it was determined that the data being analyzed were best modeled by a first-order Markov chain, and that the four groups were heterogeneous, attention could be given to interaction shifts over time. The Anderson-Goodman test (1957),

an assessment of stationarity, was run across each task for each group, and across composite group matrices. The null hypothesis is that time parameters are stationary, whereby matrices for each time period are considered not to be significantly different form the composite matrix representing all subgroup matrices. Based on a chi-square distribution, a significant test would indicate temporal change.

To test for the first research question--relational development will occur within the performance of a task--meetings for each task and group were compared with one another. For example, meetings one and two were analyzed for group one, task one; meetings one and two for group one, task two, and so on.

To test for the second research question--stages in the second task will not significantly differ from those of the first task--the composite matrices for each group and each task were submitted to statistical analysis. Specifically, the compositie for group one-task-one, for example, reflects a synthesis of meetings one and two for this particular task. This composite was then compared to the second composite, representing task two. These composities are indicants of group structure at two different periods of time.

A discussion of the results of these stationarity tests are presented in the following section.

Results and Discussion

Research Question #1: Relational development will occur within the performance of a task.

The null hypothesis for this research question was not rejected in any of the stationarity tests (see Table Two). No significant differences were found when comparing temporal shifts between meetings for each group and task, i.e. no variations in relational control were found to exist as groups engaged themselves conversationally across meetings within each task. This is rather

Table Two

Stationarity Tests Across Meetings, Within Tasks*

		Group 1	Group 2	Group 3	Group 4
Task 1	Mtg 1	11.65	8.33	5.96	7.85
	Mtg 2	17.89	10.96	7.65	6.77
Task 2	Mtg 1	4.17	21.30	7.22	6.47
	Mtg 2	8.96	31.70	9.75	5.58
	Mtg 3				10.09

* Anderson-Goodman, 20df, critical value=40.00

interesting in that previous findings have consistently discovered groups to vary in their decisioning processes across meetings within the performance of a given task. Yet, as displayed in Table Two, a prevalent lack of such structuring is apparent. As noted by Hawes and Foley (1976, p.245) when they discovered stationary decisionary parameters, the most basic explanation is that group member's order of talking and functions of utterances did not significantly change throughout task completion. Since the strings of codes were found to be of a first order nature, i.e. each act is best predicted from the utterance immediately preceding it, structuring would have been apparent by those interacts (two contiguous acts) inducing order in the interaction. Had they been found, one would then go directly to the row-by-column matrix representing each time period, and seek to account for why, for example, deference behavior (category four) followed dominance behavior (category one) more frequently in meeting one as compared to meeting two. Yet no particular utterances shifted significantly enough to qualify as first order structures. Apparently, groups did not move beyond orientation stages to more personal and controlling stages across meetings within task one.

Research Question #2: Stages in the second task will not significantly differ from those of the first task.

Similar to the findings of across meeting conversations within task one, stationarity tests for composite matrices across tasks were generally non-significant. A perusal of Table Three indicates that only Group Two seemed to differ across tasks in their conversational behavior, yet it should be noted that an argument can be made that these apparent differences could represent a statistical artifact. Of all the stationarity tests run, the only significant comparison emerged in this case. Lewis (1970) has noted, for example, that such inconsistent results are best interpreted as inconclusive and should not be employed to make claims about significant stationary parameters, i.e. phasic shifts over time. Such a lack of patterning thus suggests that accounting for a single significant case should only be used to ask additional empirical questions until additional data is collected.

Table Three

Stationarity Tests Across Tasks, Across Groups*

	Group 1	Group 2	Group 3	Group 4
Task 1	18.50	73.14**	7.55	13.58
Task 2	21.60	57.62*	10.23	7.09

* Anderson-Goodman, 20df, critical value=40.00 (p .05)*
63.69 (p .01)**

Here it is seen that in all but one case (Group Two) the null hypothesis was not rejected when analyzing group structures (via composite matrices) across tasks. Group members, acting as a collectively, conversed similarly when task interaction was compared. Therefore, the expectation that the second task would not promote differing stages or structures was confirmed.

These results are at once understandable yet puzzling. First, it must be stressed that quite possibly the nature of the groups and the tasks led to an overall lack of phasic progression and relational development. The groups employed in this study were members of a class that had no prior interactional experience with one another, may or may not have taken the tasks seriously, and quite possibly did not interact for a period of time sufficient for major patterns of conversational behavior to emerge. When studying groups such as these, it is necessary to realize that they often feel forced to interact with one another for purposes of class evaluation and grading. And often times grades per se are not impetus enough for students to personally invest themselves in task accomplishment. The results can lead to an overall lack of concern with group development, and instead an overriding attitude of "Let's get done with these assignments so we can go enjoy ourselves." Since only four groups were studied, it is highly possible that such a predisposition prevailed. Only additional inquiry can further determine whether or not classroom decision-making groups tend to remain stationary in their conversational sequencing.

Another related explanation that is also understandable concerns the duration of relational interaction across tasks. I would conclude that neither of the four groups interacted longer than four hours, if they conversed even that long. Should distinct phases of interaction be expected to emerge in such short periods of time? How long does it take for individuals to move beyond orientation stages into those of conflict and personableness, especially when such motivation may not be present? Knapp (1978), for example, discusses the tendency for persons to linger in the "experimentation" stage for varying periods of time. This stage is

recognizable due to the prevalence of "small talk" and overtly uncritical discussions. It is very likely that interaction across two short tasks would not promote further exploration into what Knapp (1978) describes as "intensifying" stages, and group researchers such as Fisher (1970) and Tuckman (1965) have termed "conflicting" and "storming" stages, respectively.

On the other hand, it is puzzling that groups could accomplish two tasks--one externally oriented, and the other self-analytic in nature--and fail to shift the nature of their relationships with one another. Task accomplishment presupposes some type of movement if in fact groups wrote a short paper together (task one) and later presented their findings of listening to themselves on recordings of their first task (task two). Each group did engage in these activities, and four papers and presentations did emerge from these group performances. What then can be concluded about the lack of temporal change across meetings within the same task, as well as the stationarity of groups across different tasks? I suggest that one viable response to this question rests within the coding activities themselves, as practical accomplishments requiring constant decision-making on the part of the researchers. Another concerns how conversation is conceptualized within a relational coding framework. Both issues are confronted in the final section of this investigation.

III. Coding and Conversation?: Reflexive Considerations

What About Coding?

It is typical in relational coding studies to argue that if high reliability coefficients are attained among coders, validity claims are forthcoming. Therefore, a simple reporting of these coefficients

is sufficient, along with a short description of the category system employed in the study. If one were to only consider these aspects within the previously reported study on stages of group development, there would be little choice but to rely upon the empirical findings as "true" or at least a viable representation of group behavior. However, a different set of perspectives can be employed to make sense of the lack of non-stationarity in this study.

How is coding done, and what impact do these performances have on these empirical findings concerning stationarity? Put simply, the codes of the tapings are the data for the study. Consequently, how the codes are generated will determine any knowledge claims about group behavior. The linkage between codes and outcomes, then, is most crucial. But what attention has been given to the practicalities of coding in relational coding work? A consideration of the views of educational researchers and ethnomethodological reports provides an apt comparison.

Within the educational field (and others as well), interaction analysis techniques are employed in studying patterns of classroom behavior. If teacher-student interaction patterning can be identified, useful conclusions can be drawn as to the impact of teaching performance on student achievement (see Rosenshine, 1971). Yet the diversified steps required to accomplish these research procedures are constantly being scrutinized and refined for purposes of systematically validating coding procedures (e.g., Frick and Semmel, 1978; Munby and Wilson, 1978). If high reliability is attained, to what extent can it be argued that such statistical correlations reflect the validity of interpretation? McGaw et al., (1972), for example, stress the fact that percentage of agreement does not necessarily imply accuracy of judgment. Coding errors must then be considered likely sources of

variance, as are those phenomena being observed. Behaviors, especially conversational ones, are often enacted in ways that are problematic for coder identification: Our actions are not always identifiable according to mutually exclusive and otherwise "clean" categorical descriptions. Ambiguities can be expected for numerous reasons. Not only can utterances serve multiple functions during talk--and it is often difficult to decipher the most "dominant" function (see Wooton, 1972)--but the naturalistic occurrence of talking is such that basic elements occur: Talk-overs; interruptions; simultaneous utterances; and hard-to-hear utterances due to background noises and recording difficulties. In the present study, coders complained of having a difficult time coding directly from audio-tapes. Conversation in its naturalistic form occurs at so many different "levels" that the coders had to generate mutual rules during training for identifying and coding certain simultaneous utterances while overlooking others. Such decisions are not easy ones to make, nor do they necessarily do justice to the talk per se. How coders generate these rules--guidelines that are not nor could not be included in a coding manual--has been of interest to a group of researchers investigating "coder drift" (e.g. Kugle, 1977; 1978; Marston et al., 1978; Zimmerman and Kugle, 1978). In attempting to explain inconsistent findings in classroom settings, they argue that not only do systematic differences exist as coders rate same teacher behaviors, but this occurs because coders create their own idiosyncratic systems of classification when working alone and in pairs.

Similarly, Garfinkel (1967, Ch.1) and his colleagues discovered that as coders attempted to follow a priori "coding rules", they had little choice but to rely upon their own strategies of making sense while choosing among coding alternatives, and thus engaged in several <u>ad hocing</u> procedures that better allowed them "...to grasp the relevance of the instructions to the

to the particular and actual situations they are intended to analyze." (p.22). Relying upon their native competence as speakers and part of the research "arrangement", coders were required tobe active in their decisioning according to "proper" decisioning rules. The criteria for "proper", however, are often left unexplicated in social science research. One conclusion that can be drawn here is that, ironically, what science gets out of coding is entirely dependant upon those priorities the coder's themselves utilize when identifying behaviors. Since different individuals possess and employ different criteria and priorities, however, this becomes another problematic issue concerning coding.

The generalizability of coding findings is a relevant consideration in light of coding problems. Frick and Semmel (1978) suggest this is frequently the case because utilizations of coding systems are highly related to how those who originally developed the scheme interpret the rules and subsequently train coders in its usage. A perusal of two representative contingency tables for the previously described groups study, as seen in Appendix A, provides a very specific example. In the present study, one highly probable reason to doubt the empirical findings is because coders consistently identified "two" and "three" utterances, possibly at the expense of discriminating among other types of control utterances. Although they were proven reliable, their identification procedures were possibly inappropriate, even though to them (and their coding trainer, who had become reliable on the system previously) many utterances could be identified as structuring and equivalence response modes.

But the assumption that "appropriate" identifications exist when engaging in relational coding is less important that inquiring as to

how assumptions of discrete-coding and probabilistic structures impose unnaturalistic constraints on conversation. If the reader takes a careful look at the categorical descriptions provided on page fourteen of this report, it can be seen that how we talk in our daily interactions varies considerably. As a practical activity, conversation is managed through various elements and structures (e.g. arguing, joking, laughing) that are glossed when coded for specific functions such as five categories of relational control. This need not be a weakness of relational coding if coding per se were not vulnerable to "errof", but this previous discussion would suggest otherwise. In fact, the argument could be posed that the empirical necess ties of the Markov model discount the intricate displays of relational control in our daily lives, and it is for this reason that O'Keefe, Delia, and O'Keefe (1978) suggest that coding schemes such as the one employed in this study are at best "abstractive templates" that inadequately capture naturally organized features of conversation. Or, as I have heard it stated previously, trying to explain the nitty-gritties of communication via relational coding is like "Taking an axe to a spider-web."

By looking at the study reported herein as a set of practical accomplishments, as steps that make a major difference in discovering patterns of human communication, it must be concluded that only through close inspections of these moves can communication researchers ever hope to provide rigorous and insightful accounts of social events. To ask "What's so relational about relational coding research?" thus becomes a very legitimate inquiry. Such a question leads one to reevaluate, as in the example of this report, what "significant" results <u>are</u> if basic presuppositions are continually overlooked. Hopefully, this investigation has drawn attention to the need for such a reevaluation process.

Footnotes

1

For those readers who are and are not associated with ethnomethodological work (a matter of degree and type of recognition), it is important at the outset that I clarify my usage of the term. My utilization of Garfinkel's views on the reflexive nature of activities, social and research alike, is a borrowing of but a small portion of a decidedly more encompassing orientation. Within this orientation, the ideological split between Garfinkel and Cicourel is a notable one, and for my present purposes I am associating most strongly with Garfinkel's views on research activites as practical accomplishments that, when viewed common-sensically, can lead to different understandings of what certain methodologies can and cannot inform us about the social world.

This application should not be interpreted to imply that interactional research--The Interactional View itself representing a diverse group (see Footnote Two)--is in any way "inferior" or "less rigorous" than ethnomethodology per se. Rather, inherent in Garfinkel's views is a (sometimes implicit, sometimes not) call for the need to render as problematic those accomplishments that are crucial in all scientific work. Data collection procedures are only one set of activities, and the argument I am proposing suggests that there is reason to believe that a rather critical inspection of interactional methods leads to relevant questions currently glossed in our research reports. Ethnomethodology can thus become a valuable resource for critically viewing our work as a topic of interest.

It is in this very limited sense that I am proposing a reflexive tie between interactional and ethnomethodological assumptions, an interface that suggests both similarities as well as differences in terms of scope, application, and empirical status. However, a critical and detailed synthesis of this kind extends far beyond my present concerns. In my particular case, ethnomethodological

readings have proven useful for constructing explanations regarding the practicalities of doing interactional work, in relation to the outcomes of these activities in the form of knowledge claims about human communication. I am thus drawing attention to the interface of interactional methods and conceptualizations, attempting to presuppositionally question the stated goals of the research in light of how we go about achieving these ends. My assumed task is a perspective-taking one, designed to hopefully provide a more thorough understanding of the importance of observing how we go about observing, and not simply taking these issues for granted in our scientific inquiries.

2

The recent ASILOMAR Conference--a gathering of communication educators, researchers, and psychotherapists--provided an opportunity to celebrate the work of Gregory Bateson. During the conference, it became obvious to many if not all of us that interpretations and accounts of what the Interactional View "really" was (the determination of which is an impossible task, of course) varied considerably. This differentiation was evident not only among members of our discipline, but also within the colleagial framework of the Palo Alto Group, and across these factions as well. Consequently, when I suggest that intimate ties exist with the Interactional View, it must be realized that the impact within our field is somewhat diversified, and that many of the substantive issues I raise in this report may or may not reflect those others whose work has alsc been influenced by the Palo Alto Group.

3

I chose not to include in this report many of the issues which can be raised about having groups record their own conversations. While the argument could be raised that such recording significantly impacts and even distorts, I can only note that my experience has been that this is not the case. Groups very quickly become accustomed to the presence of recorders, and have repeatedly

confided that they would prefer such openness and candor rather than being recorded without their awareness. Many ethical problems can be raised in light of "secret" recordings, and these can be avoided when first requesting permission for recordings, and next instructing groups as to how "hearable" recordings can be made for any of a number of purposes.

4

The many details of these procedures have also been deleted from this report, yet will be discussed in part in the final sections of this investigation. Suffice it to say that, in the doing of this empirical study of group systems, a naturalistic study of how these coder's worked was simultaneously conducted. The results of this coding inquiry are being reported elsewhere, but in general do raise serious questions as to the believability of findings generated via relational coding techniques.

Appendix A

Examples of Categorical "Loadings" in Contingency Tables

Example: First Order Contingency Table for Group One, Meeting Two

Antecedent States	Subsequent States: One	Two	Three	Four	Five	Total
1	.4000 2	.4000 2	.2000 1	---	---	5
2	.0153 3	.2602 51	.5612 110	.1633 32	---	196*
3	--- 1	.4355 108	.4597 114	.1048 26	---	248*
4	.0141 1	.4930 35	.3099 22	.1831 13	---	78
5	---	---	---	---	---	0

*Note the prevalent lack of one, four, and five codes. Why did coders "see" more twos and threes consistently?

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